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Canadian Journal of Psychology

PREDICTION IN CLINICAL PSYCHOLOGY[1]

C. R. MYERS
University of Toronto

INTRODUCTION

DURING the past year, I have come to think of the President's Address by its initials: "PA." These initials seem, to me, peculiarly appropriate to the occasion. Perhaps that is because, in the RAF, the symbol "PA" means that the papers so marked are now ready to be "Put Away"—filed, lost, locked up, or otherwise disposed of. And so it is with the President and his Address. The time has come to "PA" them both.

Like many of the tribal customs which characterize both primitive and professional societies, the President's Address is a singularly self-punishing ceremonial. Some few of you know to your cost what it is like to prepare one. The rest of you know, also to your cost, what it is like to listen to one. Consequently, I must join my eminent predecessor in this office, Dean Chant, in wondering audibly why we keep on having the things.

Of course there was a time, in the early days of our North American tribe, when the President's Address had some chance of being more than a hollow gesture. Looking back now from mid-century, through the magnifying haze of forty or fifty years, we can almost hear again the stirring calls to duty, the thundering challenges, the alarums and excursions of those earlier days. As we listen to the silver tongue of Angell (1906) announcing the Birth of Functionalism, or the brassy boasts of Watson (1915) proclaiming the Behaviourist Manifesto, we get the impression that Presidents in those days really had something to say. Right or wrong, good or bad, it now seems as though it must have been at least worth the price of a meal to have been there.

But now—let us face it—times have changed. No one seems to want to start a large-scale battle in psychology any more. The general common ground on which we all meet seems to be pretty well cleared and settled and quiet. Of course there is always fighting on the frontiers, as there is in any healthy science, but these hot little controversies are mostly private fights between specialists. We no longer understand the problems well enough in any special branch of psychology other than our own to know, really, what all the fighting is about. We must, perforce, leave

[1]Presidential address, delivered at the Annual Meeting of the Canadian Psychological Association, Toronto, May 19, 1950.

most of this jungle fighting to the fox-hole experts who are specially trained for it.

And so Presidents' Addresses have fallen on evil days. The clarion call to battle has now become merely a "Swan Song," and the Seashore Musical Talent Profile of the average swan is said to be not only low but exceedingly flat as well.

There is, however, one characteristic of the President's Address to which we cling with grim determination. It may or may not be dull. It may or may not be significant. But whatever else happens, it *must* be serious. Since even he who scoffs at tribal ceremonies must still, himself, be buried some day, permit me then, with all due solemnity, to bury myself in my own PA.

Psychology in 1950

The species, "Psychologist," as it is found in North America, has undergone profound change within the last decade. In the first half of that decade, the majority of psychologists were more or less abruptly removed from their laboratories and classrooms, and found themselves in the embarrassing position of having to show what they could do to help solve some of the urgent and intensely practical problems of a society at war.

That they acquitted themselves rather well on many strange jobs, in many strange places, is less important for our present purpose than the fact that they, themselves, were changed by the jobs they had to do. Others may have been surprised sometimes to find that a psychologist could be useful, but no one seems to have been more surprised at the effectiveness of his methods than the psychologist himself.[2] Psychologists found, not for the first time to be sure, but on a much larger scale than ever before, that they had a way of studying people which was much more powerful in practical settings than had previously been supposed.

The consequence was that when these same psychologists returned from the temporary digressions of World War II, *they* had changed. They had an appetite for doing things. They had greater confidence in their methods. And they had a heightened sense of their social responsibility. Along with these "homing" psychologists came large numbers of alert, relatively mature, and very intent veteran students who, for one reason or another, shared this confidence in, and respect for the application of scientific method to human affairs.

[2]He discovered, for example, that coefficients of correlation so low that they would have been discarded as worthless in his pre-war laboratory, were in actual practice sometimes capable of being used in such a way as to save vast amounts of time and money.

It is small wonder then, that the face of psychology in North America has changed. The nature and extent of that change in the United States is too well known to require much elaboration here. There has been a sharp increase in the demand for "applied" psychologists of all kinds. There has emerged a new and vigorous profession of "Clinical Psychology." It is symptomatic of the change that, in June, 1949, one-third of the entire membership of the American Psychological Association named the Division of Abnormal and Clinical Psychology as their "first-choice" among the twenty sub-divisions of that Association.

The fact that parallel changes have taken place in Canada is not so well known. The Canadian Psychological Association is about one-tenth the size of the APA and is, of course, still very young. Yet changes in the occupational distribution of our membership within the decade reflect the same trends to about the same extent.

Comparing our present (May, 1950) membership with that shown in our first published list in 1942, we find that the total membership has risen from 118 to 618, an increase ratio of 1:5. If we classify the names on each list in terms of occupation, we find that this increase has been disproportionately high in the applied, and especially in the clinical areas.

Disregarding "students" and cases in which occupation is insufficiently specified, the remaining names on each list can be classified broadly into "full-time clinical," "part-time clinical," and "non-clinical" occupations.

Whereas in 1942 only 16 of our members were engaged in full-time clinical work in hospitals, clinics, institutions, and agencies, now 138 of our members are so engaged, giving an increase ratio of nearly 1:9.

Classifying as "part-time clinical" those of our members engaged in school teaching, guidance, personnel work, University clinical teaching, and consulting and practising psychologists, we find that in 1942 the number of our members thus engaged was 32. Today the number is 208, an increase ratio of 1:6½.

In 1942, we had 44 "non-clinical" members (mostly university teachers with non-clinical interests). In 1950, the number of these has risen merely to 74, an increase ratio of less than 1:2.

Today, one-third of the CPA membership is engaged in full-time clinical work. Four out of every five of our members are engaged in either full-time or part-time clinical work. Such facts reflect a substantial change in Canadian psychology. Since federal support for clinical training programmes in this country is very recent, this trend toward clinical applications is all the more impressive. Furthermore, there does not seem to be any reason to anticipate an early reduction in this trend.

It must be admitted that any further shift of psychologists in the direction of clinical preoccupations, either here or in the United States,

might justify the fear that an over-grown tail is about to wag the psychological dog right off his feet.

Clinical Psychology in 1950

The fact is, however, that the analogy of the "tail-wagging dog" is rather less appropriate than that of the "tail-swallowing snake." For just when so many psychologists are turning clinical, clinical psychology itself, it seems, is turning psychological. One reason for this is that the tremendous demand for increased training facilities in clinical psychology has drawn into that field large numbers of mature and able psychologists from other branches of the discipline. Their effect is now becoming apparent. They have brought with them the sceptical outlook and the "hard" methodology which they learned in other areas.

The strength of this "scientific" trend in clinical psychology was impressively evident, in August, 1949, at the Boulder Conference on Graduate Instruction in Clinical Psychology. This two-week conference, sponsored by the United States Public Health Service and organized by the American Psychological Association, was attended by representatives of each of the forty-two graduate departments offering recognized Ph.D. programmes of training in clinical psychology. Thanks to the availability of federal support for the training of mental health personnel in Canada, the President of the Canadian Psychological Association was able to accept an invitation to participate.

There is no doubt that the Boulder Conference was held at a very opportune time. Immediately following World War II, there was a heavy demand in the United States for professionally trained clinical psychologists. This large-scale demand came first from a re-vitalized Veterans' Administration, and later from the United States Public Health Service. It resulted in an almost feverish period of expansion and reorganization in many of the major university departments of psychology. Unlike their colleagues in psychiatry and psychiatric social work, psychologists had no well-established pre-war tradition to follow in developing professional training. Thus, goals and policies had to be formulated; staff and facilities had to be expanded; hordes of high-quality graduate students had to be absorbed; all at top speed.

By 1949, departments of psychology had begun to settle down. The acute emergency had subsided. There was time now to look around, compare notes, and find out how other departments had coped with *their* emergencies. Indeed, it seemed high time that those responsible for training in a new profession should have a proper opportunity to reach agreement on just what sort of a new profession was needed, and what sort of training would best contribute to its future development.

In his address at the opening session of the conference, Dr. Felix,

Director of the National Institute of Mental Health (USPHS), stressed the essential dependence of clinical psychology on the research progress of psychology generally. Speaking of the potential contribution of the clinical psychologist in mental health work, he said:

> The service which he renders to the community in which he works is dependent upon the contributions to the field made by the social psychologist, the physiological psychologist, the experimental psychologist, working on those basic fundamental problems which may not at first seem to bear directly upon the problem. . . . of the mental health worker.

This opening emphasis on the importance of basic research turned out later to be much more than a mere expression of a laudable sentiment. The Boulder group themselves were to decide eventually, though not without much difficulty and debate, that the snake had better swallow his tail, the psychologist had better absorb the clinician.

The final agreement that all clinical psychologists must be trained for research, no less than for service, was a surprising and significant event. Initially, opinions on this question were widely divergent. Throughout the discussions, contrary views were given very full expression. Is this training emphasis on research really desirable? Is it necessary? Is it feasible? The "service" advocates deplored the time-cost involved for future practitioners. The "research" advocates were equally opposed and stressed the alleged rarity of the true "research mind." Yet slowly, and painfully, and reluctantly, by a process not unlike non-directive "clarification," the group arrived eventually at an astonishing degree of unanimity. All clinical psychologists must be research-oriented and research-trained. They will not all devote themselves primarily to research. But they must all have a high level of competence for they must all be able to stimulate, supervise, and evaluate research. More and better clinical research is the first and basic need of the emerging profession of clinical psychology.

This is one of the many policy questions where analogies to the medical profession can be very misleading. With its long and honourable tradition of service to humanity, the medical profession now has a vast store of dependable knowledge which it can use in daily practice. Thus, a relatively small group of highly trained specialists can be assigned to the task of research. Without such a long tradition of service, and without such a store of well-established knowledge, the clinical psychologist must do "front-line" research. He must have his supporting scientist, not behind him, or even beside him, but *in* him.

It seemed to those at Boulder that the future of clinical psychology is both promising and precarious. It is promising because of the wide current demand for clinical psychologists and the immense unserved needs of our society. It is precarious, however, because clinical psychology

has yet to demonstrate its social usefulness. There is now sufficient public and professional faith in its potential usefulness to provide ample opportunity. But what emerged from the discussions was a sober recognition of the fact that, though society may need the services of clinical psychology, what clinical psychology needs most urgently is more dependable knowledge and more valid methods with which to do its job.

A candid examination of the superstructure of clinical practice in the mental health field today forced on the conference reluctant agreement that a much firmer research foundation must be built. Clinical psychologists have a distinctive responsibility for this because of the traditional research orientation of their discipline.

The official report of the Boulder Conference will appear in print shortly. It is a document of some importance since it will be the preliminary blue-print of training policy for an emerging profession. It has, of course, (and it needs) no other authority than the fact that it reports the agreements and disagreements of those responsible for the training of future clinical psychologists.

Let us turn now from the Boulder conclusions and take a brief look at the present situation in clinical research.

Clinical Research

It would appear that the most urgent problem confronting clinical psychology in 1950 is the validation of its own current theories and techniques. Most of our clinical assumptions and methods have not been validated at all. Many of them have simply been borrowed from prestige sources in psychiatry and psychoanalysis. The clinical psychologist will not survive if he is to be no more than a half-baked psychiatrist or an unanalysed analyst. He has a really valuable contribution to make only if he can apply his distinctive scientific training effectively in the clinical area.

The post-war battle of the clinical bulge in psychology is just about over. The excitement is dying down. Now comes the hard grind of scientific validation. The clinical psychologist is going to have to pay his own way in the society that is offering him his present opportunity.

Are the methods of psychologists any more effective than those of common sense? Do they yield any more dependable results than the "intuitions" of experts? Frankly, the evidence in the clinical area is yet to be supplied. In certain other applied areas, such as personnel selection, psychologists have been able to show that their methods are measureably superior to those of common sense or expert opinion. It is my opinion that, in the clinical setting, comparable evidence will only be obtained in essentially the same way, namely by the systematic re-

cording of observations, on the basis of which to make unequivocal predictions which can then be tested for accuracy.

It would be quite incorrect to imply, however, that the application of this familiar method in the clinical setting is going to be as easy as it sounds, or even that it will be no more difficult than it has been in other applied areas. The absence of an impressive research literature in clinical psychology at least suggests that there are some rather special difficulties in its application to this area.

The most obvious, if not the most important, of these difficulties is the clinical atmosphere. By its very nature the clinical setting is strongly biased in favour of immediate service and against long-range research. Our professional colleagues, the psychiatrist and the psychiatric social worker, have been reared in a basically "service" tradition. The welfare of a human being is always at stake and this factor inevitably dominates the value system. Thus, all pressures are in the direction of immediate practical action rather than prolonged, critical thought. Indeed, there is a premium on the confident, rather than the sceptical, outlook.

To state these difficulties, is not to object to them. But they do need to be recognized by the clinical psychologist who proposes to try to be scientific. He cannot take his "specimens" off to a quiet laboratory, there to create an atmosphere more congenial to unhurried methodical analysis. For it is precisely *in* the clinical atmosphere that we need more valid methods of examination, appraisal, and therapy. And in that atmosphere the psychologist is not usually free to employ his accustomed experimental weapons.

If, however, we go right back to our own first principles and reexamine conventional scientific method in its most elementary form, we will, I suggest, find that it *is* possible to be scientific within the clinical atmosphere. The old familiar method evolved through the slow centuries from the first harmless astronomers down to their more alarming atomic descendants, goes something like this:

(1) *Hypothesis.* The observer formulates an hypothesis on the basis of his accumulated observations and his background of general knowledge. This is merely his "best guess" in terms of what he has thus far learned.

(2) *Prediction.* He then makes a prediction of a defined type: (*a*) It is sufficiently explicit that he can later tell whether he was right or wrong, and, (*b*) he specifies the grounds upon which the prediction is based.

(3) *Verification.* Then he observes carefully to determine the accuracy of his prediction.

This is the simple pattern, but it does not end there, for the pattern is a spiral. Examining especially his errors in prediction, he revises his hypothesis, and tries again and again and again.

It should be noted that it is precisely in situations where the observer has relatively little control over the events in question, and where an "experiment," in the usual sense, is not possible, that this ancient method is most useful.

Now it is a curious and, I believe, an important fact that the clinical psychologist seldom makes any prediction at all. And when he does predict, he seldom checks to see whether it was right or wrong. And if he does check, he usually discovers, (*a*) that his prediction was too vague for him to be sure whether it was correct or not, and (*b*) that the grounds for the prediction were not very clearly specified anyway.

You may feel that this statement is too severe, and perhaps it is. The point of importance, however, is that unless we satisfy certain of the elementary requirements of the method, we cannot expect it to work. It would seem that, in the clinical setting, we are forever taking the first step—guesing—without being able to (or perhaps without daring to) follow it up with the next two steps—predicting and verifying. Having tried, myself, to use this "prediction-test" method in clinical work, I am not unaware of its difficulties. But I am convinced of the value of the effort and concerned that attempts to use it are not more prevalent in clinical psychology.

Prediction

The *Psychological Abstracts* for 1947, 1948, and 1949, taken together, include a total of 16,810 titles. A search of the subject index in each volume under the entry "Prediction," reveals a "grand" total of 24 titles listed under this heading. While it may be true that prediction studies are, in fact, more frequent than the 1.5 per 1,000 which this suggests, nevertheless it seems fair to conclude that such studies are not prevalent in current psychological literature. (Of these 24 references, incidentally, there were 16 in 1947, 5 in 1948, and only 3 in 1949. It would not appear that they are increasing in frequency either!)

The references themselves are instructive, however. Most of them have to do with the prediction of success at school, or in college, or in marriage, or on parole.

There are two studies (1, 5) on predicting success in pilot training which are relevant here because they are concerned with the use of the "clinical interview" for that purpose. The results indicate that the interview adds nothing to the accuracy of predictions based wholly on objective test methods.[3]

There are two more strictly "clinical" studies which may be mentioned briefly to illustrate the potential value in the use of this method. Tolman

[3]It appears, from this and other similar studies, that our present confidence in the "clinical interview" is in for a thorough shaking.

(8) reports the outcome, six years after a clinical psychologist had made explicit predictions of success or failure on parole for a group of 193 delinquents. The predictions were confirmed in 70 per cent of cases. It should be noted that particular importance is attached to the re-examination of the records in cases where the prediction was *not* confirmed by outcome.

Harris and Christiansen (2) report the results of an effort to predict response to brief psychotherapy. Fully recognizing the complexity of the factors involved here and the limitations which must be placed on the interpretation of any single study, it is still of interest to note the crop of stimulating and testable hypotheses which emerge from such an effort at systematic prediction. Is it true that the outcome, in such cases, is *not* related to the patient's level of intelligence? Are there really specific minimal signs of reality distortion which give dependable advance indication of outcome?

My main point here, however, is that prediction is not widely used in psychology generally, nor in clinical psychology in particular. If it is not, we do not have far to search for the reasons.

It is probably a mistake to teach beginners that the prediction of human behaviour is one of the goals of psychology. We end up by believing it ourselves! Making prediction a goal puts it well up on a very high shelf. It becomes one of those laudable but distant and probably unattainable objectives which no one takes any more seriously than the banner on a political platform.

There are good reasons for thinking of prediction, not as a distant goal, but rather as an important part of our present operating method. As MacLeod (6) points out, "The goal of science as science is not prediction and control but understanding. Prediction is merely the test of understanding and control the practical reward" (p. 209). If then, prediction is really the ultimate test of validity in all scientific endeavour and if validation of current theory and practice is the most urgent need in clinical psychology, it is high time we made more deliberate use of prediction as a means of increasing the precision of our clinical understanding.

There is, however, another and a deeper factor which operates against the use of prediction in clinical work, and that is the lurking suspicion that it is really not possible. This suspicion stems originally from the infamous "man-in-the-street" (including you and me) who likes to think of himself as a free agent and resents the very suggestion that anyone could predict *his* behaviour. Of course, he realizes that his wife's behaviour is highly predictable under specified conditions, and he is often amazed at the uncanny accuracy with which she seems to know what he is going to say or do long before he, himself, has "made

up his mind." No, the predictability of human behaviour is not really questionable. On the contrary, it is positively shocking!

And yet it is true that, in psychology, we have had most of our experience (and most of our success) in predicting *group* behaviour. Accordingly, since our familiar statistical devices are group-centred rather than individual-centred, they tend to leave the clinical psychologist stalled with data which have been derived from a single "unique" case. The recent symposium on "Statistics for the Clinician" (9) gives grounds for hoping that many of these technical problems are on the way to solution.

Of more immediate importance, however, to most of us engaged in clinical work are the ordinary practical difficulties of using prediction effectively as a routine procedure for the purpose of improving our own understanding of patients, and thereby our skill in helping them.

Some effort to encourage the prediction habit with students commencing interview training has shown that, although it is difficult, it is also rewarding. Through the use of recordings, it is possible to stop the interview at a predetermined point and ask for an immediate prediction (What is going to happen next? How does the patient feel about this? What will the interviewer say now?) and then move right on to test the prediction against what actually follows. Similarly, predictions from one recorded interview, concerning the subject's subsequent feelings and behaviour, can be checked against the recording of the next interview.

In this process, it becomes apparent quickly that the initial accuracy of predictions is less important than the early opportunity to verify them. It also appears that aptitude for this type of predicting in students is correlated with other (supposed) measures of clinical sensitivity and insight.

One of the by-products of this procedure is both amusing and instructive. Students who have some previous familiarity with the literature on interviewing often find it difficult to believe at first that the recordings of perfectly genuine interviews have not been "staged." They seem to feel that real people should not conform so slavishly to the textbooks! There seems little doubt that the uniformities in human behaviour are impressive enough to permit considerable accuracy in prediction even now.

There is much that we have, or should have, learned from previous experience in personnel selection where prediction is the commonly accepted test of validity. Much of this experience is almost directly applicable to clinical prediction. These lessons and their relevance for the clinical psychologist are admirably summarized in Horst's (4) monograph on *The Prediction of Personal Adjustment.* The general ra-

tionale of clinical prediction has been well outlined by Sarbin (7). These carry us well up to the frontier of clinical validation. From there on, it seems to me, we have a great deal of groping to do before we can know how best to apply the prediction method in clinical training, practice, and research.

Meanwhile, there are at least a few elementary pointers which, although obvious enough, are so commonly overlooked in current clinical practice that I venture to enumerate them:

(1) The purpose in developing the habit of making clinical predictions is to improve one's own understanding. Hence the spiral pattern of observe-guess-predict-check-observe-guess-predict-check, and so on, needs to become a routine habit in clinical work, not just a device reserved for self-conscious "research projects."

(2) Clinical predictions which are merely "blind guesses" are of no value. There must be reasons for the prediction and these reasons must be explicitly stated and recorded. "Intuitive" predictions which may prove to be correct, but which cannot be accounted for, add nothing to understanding.

(3) Clinical predictions which are phrased in such vague and qualitative terms that it is later impossible for anyone to know whether they were right or wrong, are worthless. Such predictions may prevent the embarrassment of being wrong, but they also prevent the possibility of learning.

(4) Clinical predictions, to be useful, need not be publicized. Indeed, there are usually very good reasons, both professional and scientific, why they should not be communicated to the patient. They should simply be recorded in a form which will permit subsequent checking.

(5) There is no value at all in making clinical predictions which are *not* subsequently checked against outcome.

(6) Assuming that clinical predictions have been made and checked, the greatest learning value is to be derived from a careful examination of *errors*. Correct predictions may be based on irrelevant reasons. But incorrect predictions not only suggest the need to revise the hypothesis, but can also suggest the sort of revision needed.

(7) Clinical predictions should not be exclusively concerned with long-range outcome. Short-term predictions are not only easier, but generally more profitable. They give much more opportunity for prompt checking and revision.

It may be added that, in attempting clinical predictions, a determinant of major importance is the patient's "idea of himself." Clinically, the most significant predictive clues are likely to come from an attempt to understand his "private" world.

Conclusion

The difficulty of clinical prediction is certainly impressive. It remains only to say that its difficulty is no excuse for our failure to try it. If it is the best test of validity, let us find a way of using it effectively.

Nor have we any real grounds for discouragement. We in psychology are an impatient lot. We seem to think that uniformities in human behaviour should promptly reveal themselves if we but wave our statistical wands and stir our data thoroughly with the latest model of mathematical "Mix-Master."

It appears to have taken the astronomers quite some time to find and formulate the uniformities in the skies which they, and we, now take for granted. According to Hogben (3): ". . . . ten or twenty thousand years—perhaps more—may have been occupied in scanning the night skies and watching the sun's shadow throughout the seasons. Mankind was learning the uniformities which signalize the passage of the seasons, becoming aware of an external order . . . ." (p. 22).

Mankind has not been looking for uniformities in human behaviour for ten or twenty thousand years. Indeed, it was only yesterday that he found the necessary leisure, and relief from more pressing affairs, to look at himself at all. It could be that we have quite a long way to go.

If we, in the sciences of man, are still "scanning the night skies" of human behaviour in search of uniformities, it is small wonder that the clinical psychologist cannot predict tomorrow's behaviour of John Doe any more accurately than the weatherman can predict tomorrow's weather.

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A COMPARISON OF THE A.C.E. PSYCHOLOGICAL EXAMINATION AND THE CULTURE-FREE TEST

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MANY studies (for example, 5, 6, 7, 8, 10) have shown the existence of group differences in measured intelligence, but the results have been criticized because of the experientially-biased nature of the tests (1, 3, 6). In order to reduce the effects of cultural factors on test performance, Cattell (3) suggested the use of general ability tests based on knowledge of processes and objects common to diverse cultures. It is his contention that the major weaknesses of present verbal and pictorial items can be obviated by means of geometrical problems involving only the perception of relations and correlations. Anastasi and Foley (1) too, have suggested that tests designated as "culture free" may actually favour certain groups if pictorial materials are employed. While Cattell's Culture-Free items may not be truly pictorial, they may, in several instances, be interpreted as two-dimensional representations of common objects. Further, they utilize various intensities of shading between black and white. Cattell believes that these considerations do not invalidate the instrument for purposes of group comparisons; nevertheless examination of the test leads to the hypothesis that differences between groups in interest and training may introduce bias. One study (4) indicates that performance on the Culture-Free Test is affected by specific training on test materials, although to a lesser extent than on the Stanford-Binet Test of Intelligence or the American Council on Education Psychological Examination. Davis (6) has found class differences on the Thurstone Spatial Tests, attributing them to differences in experiences and interests. Cattell partly recognizes this possibility when he points out that differences between people in interest, attention, and working speed may invalidate inter-group comparisons.

While problems employing geometrical forms may be solved at the perceptual level, Stoddard (11) contends that there is no guarantee that verbal-minded individuals will not try to succeed in some non-verbal problems by verbal procedures. Brody (2), who studied the interrelations between verbal and non-verbal reasoning, and intelligence tests, found his geometrical and concrete non-verbal materials correlated -.46, while the verbal reasoning tests correlated .56 with the Otis Test. Even so, he found a multiple correlation coefficient of .80 between the various verbal and non-verbal reasoning tests which seemed to imply that verbal

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and non-verbal abilities are not independent. Such findings may indicate that test materials such as those included in the Culture-Free Test involve processes quite beyond the perceptual level.

This investigation is designed to test the hypothesis that two groups, probably dissimilar in motives and certain types of experiences, although very comparable on a widely-used measure of academic ability, will differ significantly in performance on Cattell's Culture-Free Test. Since one purpose for administering "intelligence" tests is to predict school achievement, this study will also be concerned with the relations between ability as measured by the A.C.E. and the Culture-Free Test and achievement in certain academic courses.

The subjects were students enrolled in a class in general psychology in a four-year arts and crafts college, and students in an elementary educational psychology course in a university school of education. Sixty-six of the former students had taken both the A.C.E. Psychological Examination and the Cattell Culture-Free Test; ninety members of the latter group had taken both tests. The Culture-Free Test was administered with a time-limit that appeared to be ample for most students. Raw scores on the A.C.E. were changed to percentiles by using 1947 norms for the arts students and the tentative 1949 norms for the university group. Since separate norms for men and women are provided for the A.C.E., data for the two sexes were treated separately, even though Cattell has reported no sex differences in the Culture-Free Test.

Groups were obtained by matching individuals for Q (quantity or number), L (language or verbal), and Total A.C.E. percentile scores. Cases were considered matched if their percentile scores did not differ by more than four points. The number of cases varied from group to

TABLE I

MEAN DIFFERENCE IN PERCENTILE UNITS IN FAVOUR OF THE UNIVERSITY GROUP

Scale	Men		Women	
	N	Mean Percentile Diff.	N	Mean Percentile Diff.
Q*	23	.13	21	.05
L*	24	.25	17	.53
T*	23	−.39	16	.25

*These groups do not necessarily include the same subjects, since the groups were selected on the basis of different criteria, Q scores, L scores, and Total scores, respectively.

group because of the matching requirements. The similarity in percentile scores of the groups in terms of the matching variable is shown in Table I.

The mean percentile differences between the two groups are small, and, with one exception, favour the university students. The difficulty of treating percentile scores arithmetically is recognized, but, since no difference exceeds four percentile points, it would appear that the procedure is not too unsatisfactory, and that the groups are really very similar in terms of A.C.E. percentiles on the Q, L, and Total scales.

It will be noted from Table II that neither the range of percentile scores nor the median percentiles are representative of the college population used to determine national norms for the A.C.E. In most instances it was difficult to match either very high or very low percentiles. The results to be reported may be distorted since the complete range of A.C.E. scores is not included. It may be, however, that level of A.C.E. ability is not related to the factor being investigated.

TABLE II
RANGE AND MEDIAN OF PERCENTILE SCORES

	Men				Women			
	Art Group		University Group		Art Group		University Group	
	Range	M	Range	M	Range	M	Range	M
Q	18 - 93	38	19 - 93	41	2 - 60	35	1 - 62	36
L	30 - 99	78	34 - 99	78	10 - 94	65	9 - 94	65
T	46 - 99	76	48 - 99	76	2 - 84	50	2 - 85	50

The significance of the difference between the mean scores of matched groups on the Culture-Free Test was determined by the formula $t = \frac{M_1 - M_2}{\sqrt{\frac{\Sigma d^2}{n(n-1)}}}$ with (n-1) degrees of freedom (9). Mean Culture-Free scores, t-ratios, and levels of significance are shown in Table III.

Four of the six differences are significant at the 5 per cent level or better, and all of the differences favour the students in the art college, suggesting the possibility that scores on the Culture-Free Test are to some extent related to experiential factors and interests.

TABLE III

SIGNIFICANCE OF DIFFERENCES BETWEEN MEAN CULTURE-FREE SCORES

Matching Variable	Men					Women				
		Art Group	Univ. Group				Art Group	Univ. Group		
	N	M	M	t	Level of Sig.	N	M	M	t	Level of Sig.
Q	23	52.65	46.52	2.74	1-2%	21	51.00	44.00	3.78	1%
L	24	51.25	48.54	1.14	<5%	17	51.53	45.88	2.12	5%
T	23	52.17	50.22	1.33	<5%	16	49.93	44.12	2.57	2%

It appears to be difficult to prepare culture-free tests, and probably the same is also true for culture-fair tests. Since intelligence is measured indirectly through manifestations of what intelligence does in a given culture, it would seem that the content of intelligence tests must be culturally determined. Any investigations designed to compare "intelligence" levels of groups of peoples with different cultural backgrounds with any of the existing measuring devices seem doomed to difficulties in interpretation of the results, for differences between groups may be as much a matter of cultural differences as of differences in "intelligence."

Even if there are certain inherent difficulties in making group comparisons of intellectual ability, it may be that there are no differences in the predictive values of culture-free and non-culture-free tests. Table IV shows the correlations between intelligence test scores and scores on general and educational psychology examinations.

In general, the A.C.E. scores are more closely correlated with academic success in the arts and crafts college than in the university group, especially the "L" and the "Total" scores. Since the correlations are based upon the total available sample, it is not possible to relate these relationships back to the groups used in studying differences in culture-free scores. There was very little difference in variability between the two major groups of students, except that in total score there was a difference between the standard deviations of six points. The differences between the correlations cannot be accounted for by differences in variability in A.C.E. performance. There was a great difference in variability on the psychology examinations, σ's being 9 points and 29 points in the arts and crafts and the university groups respectively. This may affect the

TABLE IV

CORRELATIONS BETWEEN INTELLIGENCE TEST SCORES AND ACHIEVEMENT

Variables	Arts and Crafts	University
Q and Psychology*	.39	.32
L and Psychology	.59	.36
T and Psychology	.53	.39
Culture-Free and Psychology	.25	.25

*General psychology in the arts and crafts college (N = 66); educational psychology in the university (N = 89).

nature of the correlations obtained, and yet the greater variability is found in the group with the lower correlations. Similarly, the results may be affected by differences in reliabilities of the psychology examinations; the Kuder-Richardson reliabilities[1] were about .80 and .90 for the arts and the university groups respectively. It is interesting to note that the Culture-Free Test correlates to the same extent in the two groups; attempting to hold a cultural factor constant has reduced the correlations to a similar lower level. In conclusion, there appears to be no particular point in preparing a culture-free test which does not differentiate between various groups and at the same time does not materially predict academic success. Further study with larger groups may reveal more about the nature of culture-free tests and their values in a given situation.

[1] $r_{tt} = \frac{n}{n-1}\left(\frac{\sigma^2 - n\bar{p}\bar{q}}{\sigma^2}\right)$

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CANADIAN SOCIAL SCIENCE RESEARCH COUNCIL

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THE EFFECT OF MAGAZINE PAGE SIZE ON IMMEDIATE MEMORY FOR ADVERTISEMENTS

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OVER the past few years a number of pocket size magazines have appeared on the market and several periodicals have reduced their physical dimensions. What effect has this reduction in size on the effectiveness of advertisements? Page for page, does an advertisement make as great an impression on the reader if seen in a small magazine as if seen in a large magazine?

A considerable body of data indicates that, with a standard page size, attention and memory value within a magazine are related to the size of the advertisement. Can the further generalization be accepted that "the larger the magazine page, the greater is the memory value of the full page advertisement?"

Two experiments bearing directly on this problem were conducted in 1929 by Cutler (1), and Newhall and Heim (2). Neither found conclusive evidence to substantiate the hypothesis that large page advertisements were superior. While both reported negatively, their findings have been criticized on grounds of inadequate sampling. In any case, this problem is worthy of a further check if only to confirm the fact that no significant relationship exists between size of a full-page advertisement and its chance of being recalled.

In the spring of 1949 Spires and Leblanc (3) repeated the essential elements of the 1929 experiments and obtained results that suggested the present study which brings a new approach to the problem.

THE EXPERIMENTS

Two sets of dummy magazines—one a large, the other a small page size—were constructed. Each contained twenty full-page advertisements interspersed with pages of solid reading matter. Order of appearance and position on the right or left hand side of the double pages were the same in both large and small size booklets. Advertisements were identical except for: (*a*) *Size*: Small magazine size was that of "Reader's Digest" and "Coronet"; large magazine size that of "Saturday Evening Post" and "Maclean's Magazine." (*b*) *Content*: In some instances the small advertisements did not contain all the content of the large although in every case the two were identical in respect to colour, principal theme, pictures, and layout of common content. Slides were prepared from photographs of the large page.

These dummies were used in three experiments.

FIRST EXPERIMENT. The first study was a survey of 101 adults interviewed in their homes during July, 1949. A commercial market research organization divided this work among four experienced interviewers who selected English-speaking respondents on the basis of sex and residential area in Montreal. Individuals were classified as to socio-economic status and on the basis of a four-point scale. Each interviewer worked with a single size dummy and no interviewer or subject was aware of the fact that another size was available.

The interviewer presented a dummy magazine to the respondent with the following instructions: "Here is a sample magazine. Would you please look through it from beginning to end, page by page. Take as long as you wish, stop at whatever interests you, spend as much or as little time as you wish on each page. Don't bother about me. I just want to watch to see what interests you."

On completion of this task, the person interviewed was asked: (*a*) To name the brands or, failing that, the products advertised. No assistance was given and the responses represent achievement resulting from unaided recall. (*b*) To select from groups of five possible brand names presented on cards the one brand seen in the dummy. This recognition test followed immediately the unaided recall portion of the study and a card appropriate to each advertisement was used.

Results from the unaided recall and recognition tests are presented in Tables I and II respectively. Responses are classified both as to the size of dummy page and as to the socio-economic status of the individual interviewed.

TABLE I

NUMBER OF ADVERTISEMENTS SPONTANEOUSLY RECALLED

Socio-Economic Status	A		B		C		D		Total	
Page Size	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large
Number of Interviewees	14	13	16	17	16	17	4	4	50	51
Mean Number of Ads. Recalled	4.9	3.3	3.6	4.9	4.6	3.9	4.0	2.7	4.3	4.0
S.D.	1.6	2.8	2.2	3.6	2.7	2.2	..	..	2.3	3.0

Comparing the mean memory scores for large and small full-page advertisements, it is evident that 8 out of the 10 differences favour the small size page. It is also evident that no consistent pattern exists when mean scores for the various socio-economic groups are examined. In

TABLE II

NUMBER OF ADVERTISEMENTS RECOGNIZED

Socio-Economic Status	A		B		C		D		Total	
Page Size	Small	Large	Small	Large	Small	Large	Small	Large	Small	Large
Number of Interviewees.....	14	13	16	17	16	17	4	4	50	51
Mean Number of Ads. Recalled .	10.1	7.2	8.9	8.5	8.1	8.4	8.0	7.0	8.9	8.0
S.D.....	2.8	4.0	3.7	4.7	3.9	4.2	..	..	3.6	4.2

view of the fact that such results are hardly to be expected, a more rigorous examination of these data is warranted. The results of an analysis of variance of data presented in Table I are shown in Table III.

TABLE III

ANALYSIS OF VARIANCE OF TABLE I

Variance	D.F.	Sum of Squares	Mean Square	F	Hypothesis
Socio-Economic Status	1	1.473	1.473	4.754	Rejected
Page Size.....	2	1.201	0.600	11.671	Rejected
Interaction.....	2	35.358	17.679	2.525	Rejected
Within Cells.....	95	665.266	7.003		
Total.....	100	703.298	7.033		

From this analysis it is noted that the f ratios are all non-significant at well below the 5 per cent level. More specifically, the f ratio between "interaction" and "within cells" is 2.5 for 95 x 2 df; between socio-economic status and "within cells" it is 4.75 for 95 x 1 df; and between page size and "within cells" it is 11.67 for 95 x 2 df. This can only mean that no discernable effect upon the recalling of brand names can be attributed either to page size or to socio-economic status.

A similar analysis of variance was made of Table II and is presented in Table IV. Here again the analysis strongly suggests that, taken separately or together, neither page size nor socio-economic status affects the responses obtained. Specifically, the f ratio between "interaction" and "within cells" is 1.42 for 95 x 2 df which produces a probability greater than 0.05. Between "within cells" and socio-economic status the f ratio is 1.83 which gives a probability greater than 0.05 for 95 x

1 df. In the same way the f ratio for "within cells" and page size is 2.29 for 95 x 2 df and this also gives a probability greater than 0.05.

Thus, this analysis of variance strongly suggests that the apparent superiority of small over large full-page advertisements results from chance factors and that the differences in mean scores when one socio-economic group is compared with another are also the result of chance.

TABLE IV

ANALYSIS OF VARIANCE OF TABLE II

Variance	D.F.	Sum of Squares	Mean Square	F	Hypothesis
Socio-Economic Status	1	32.216	32.216	1.829	Rejected
Page Size	2	15.365	7.682	2.293	Rejected
Interaction	2	24.874	12.437	1.416	Rejected
Within Cells	95	1673.092	17.612		
Total	100	1745.547	17.455		

SECOND EXPERIMENT. This was a laboratory experiment in which the subjects consisted of 44 adults. A majority, 31, were taking courses at an evening college, while the minority, 13, were personnel department managers tested at a meeting attended for a purpose unrelated to the experiment. In terms of socio-economic status most of these subjects would be rated as 'B', a few as 'A', and a few as 'C'.

The experiment consisted of the following three stages:

(1) Twenty slides, each of a full-page advertisement not appearing in the dummy, were thrown by means of a tachistoscope on a screen for 1/10 second. The subject recorded the name of the brand advertised. The mean number of correct responses was 2.2. The purpose of this portion of the experiment was to accustom the subjects to the manner of responding.

(2) The group was divided on a chance basis into two samples, one of which examined a small page size dummy, the other a large page size dummy, for 4 minutes.

(3) Forty slides were presented for 1/10 second each. These included photographs of the 20 advertisements which appeared in the dummy. As each slide was shown the subject recorded the name of the advertisement and his score was the number correctly named from among those he had seen in the dummy.

The results confirm those of the first experiment. As will be seen from Table V, the mean number of advertisements correctly named is higher for the sample which examined the small dummy. Again, how-

ever, the differences are not statistically significant at the 5 per cent level for 42 df: t for the difference between means is 1.73.

TABLE V

NUMBER OF ADVERTISEMENTS PERCEIVED CORRECTLY FOLLOWING TACHISTOSCOPIC PRESENTATION

Page Size	Small Page	Large Page
Mean Number Perceived Correctly..	10.1	7.6
S.D.	5.5	4.0
N	24	20

THIRD EXPERIMENT. This laboratory study was a variation of the second experiment. The subjects were 32 final year medical students who, on a socio-economic scale, rate as either 'A' or 'B'. Again, the experiment consisted of three steps:

(1) The groups were divided into two samples: one read a small, the other a large, page size dummy for 4 minutes. While the students knew that the experiment dealt with advertising, they were given no information as to its specific purpose or its technique.

(2) The name of a product (for example "sterling silver," "beer" and so on) was thrown on the screen for 15 seconds while the subject wrote the names of all brands of that product seen in the dummy. His score was the number of brand names correctly recalled.

(3) Twenty slides were then presented; each consisted of three or more pictures of trade names of one product advertised in the dummy. The subject was required to record any trade names he recognized and his score was the number of trade names which corresponded to brands appearing in the dummy.

Findings, as reported in Table VI, are essentially the same as in the second experiment. Thus:

(*a*) In the first part of the study there is a slight, although not significant, difference favouring the small page advertisement. The t of 0.29 for this difference between means is not significant at the 5 degree level for 30 degrees of freedom.

(*b*) In the second part, those who studied the large page dummy improved their responses so that the difference in mean scores favoured the large size page. However, this difference is not significant: 't' is 1.08 and, for 30 degrees of freedom, this is not significant at the 5 per cent level.

TABLE VI

NUMBER OF ADVERTISEMENTS CORRECTLY NAMED UNDER CONDITIONS OF DIRECTED RECALL AND SUBSEQUENT MULTI-CHOICE RECOGNITION

	Recall			Recognition		
	Small Page	Large Page	Total	Small Page	Large Page	Total
Mean No.	9.1	8.8	8.9	9.2	9.9	9.6
S.D.....	3.0	2.8	2.9	2.0	1.8	1.9
N.....	16	16	32	16	16	32

Discussion

It is evident that none of the five variations of this study of immediate memory for 20 full-page advertisements indicate significant differences favouring either the small full-page or the large full-page advertisement. Thus the investigation provides a general confirmation of three earlier experiments carried out with somewhat different procedures.

However, it has been noted that most of the comparisons between mean scores actually favoured the small advertisements. The question may be raised as to whether the consistency of such differences warrants the generalization that a small advertisement is better than a large one in spite of the lack of statistical significance in the differences. Ignoring all factors but page size, it was possible to investigate the responses to particular advertisements of persons interviewed in the first experiment.

It will be recalled that three measures of immediate memory were available for each of the 101 individuals interviewed: the number of brands and the number of products correctly named, and the number of brands correctly recognized.

The percentage of individuals who responded correctly to each large and small advertisement was determined for these three tests of memory. This permitted a comparison of the relative effectiveness of the 20 items. Advertisements were ranked from those best remembered in the small to those best remembered in the large page size dummy. As would be expected from the findings already reported the small page size dummy was more effective in most instances; however, a few advertisements were definitely superior in the large size.

An explanation of these differences was sought by arranging the actual advertisements according to memory scores and examining them for some underlying principle which would explain the order. This

analysis was attempted by 15 persons including advertising men, professional psychologists, graduate and undergraduate students. None could see any consistent variable in the advertisements; even later when told the basis of the arrangement, they could see nothing. There was also no significant correlation between preference and order of presentation of the ads in the booklet.

We are left with the fact that some advertisements, for reasons which are not evident on the basis of visual examination, are more readily remembered when seen in a small full-page size, while the opposite is the case with others. This suggests that our general findings may have been determined by the choice of the particular set of 20 advertisements. Repetition of the experiment making use of different advertisements might be worth while. However, it is most unlikely that such a study would produce conclusive evidence favouring the large size page.

So far as generalizations may be made on the basis of this work and on the investigations of twenty years ago, it may be concluded that the decision to place an advertisement in a large page size or in a small page size magazine should be based on factors other than the absolute dimensions of the page.

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EVALUATION OF THE EFFECTS OF PREFRONTAL LOBOTOMY ON INTELLIGENCE

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A COMPLETE evaluation of prefrontal lobotomy must include a consideration of the operation's effect not only on the distressing symptoms of mental illness, but on intelligence as well. That "social" intelligence is impaired is suggested in Freeman and Watts' (3) early observation that some lobotomy patients have difficulty in making adequate adjustment to social situations. Although Greenblatt and Myerson (5) imply that the blame for this maladjustment may be with the family to which the patient is returned, his inability to meet the demands of these interpersonal relationships must still be considered a deficit. Halstead (6) has reported that "biological" or "adaptive" intelligence is impaired.

Evaluation on the basis of objective tests is complicated by conflicting results. On the one hand Hunt (8), McKenzie and Proctor (12), and Lewis (10) have stated that intelligence as measured by psychometric tests is not impaired. On the other hand, when adequate pre-operative testing was carried out, such as by Koskoff *et al.* (9) in their investigation of patients with intractable pain, by Malmo (11) in his study on co-operative psychiatric patients, and by Petrie (13) in her study on accessible neurotic patients, impairment of intelligence was demonstrated. Preliminary data on patients lobotomized at a Canadian Veterans' Hospital[2] support these latter findings. Table I presents pre-morbid I.Q.'s[3] and pre-lobotomy Wechsler scores[4] obtained on five Canadian male veterans twenty-five to thirty-five years of age whose paranoid schizophrenia was of from three to seven years' duration. I.Q.'s obtained on the same tests one to three months post-operatively are also presented.

Of the six differences one is significant and another approaches significance at the .05 level of confidence. These are the comparisons between a post-lobotomy score and the pre-morbid score. These data indicate that after lobotomy the intelligence of these patients was lower than before their illness, yet was not changed from that measured while they were psychotic. It is emphasized that the post-operative testing was carried out when the patients had reached a relatively stable level

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[2]Canadian Veterans' Hospital, St. Anne de Bellevue, Quebec. Operations performed by Dr. Harold Elliott according to the standard Freeman-Watts' technique.

[3]Based on a table of correlations between the Army "M" test and Wechsler Intelligence Test scores devised by the senior author in connection with assessment of intelligence of soldiers in the Canadian Army overseas.

[4]Dr. Dufresne, DVA Psychologist in Montreal, and his staff kindly made available their records on the Wechsler-Bellevue.

in the course of their improvement. In every case psychiatric opinion was that there was substantial remission of their illness.

TABLE I

PRE- AND POST-LOBOTOMY SCORES ON "M" AND WECHSLER INTELLIGENCE TEST

Patient	Pre-Morbid Score "M" I.Q.	Pre-Lobotomy Wechsler I.Q.	Post-Lobotomy "M" I.Q.	Post-Lobotomy Wechsler I.Q.
	(A)	(B)	(C)	(D)
A.M.	120	109	110	114
A.L.	105	71	85	86
W.C.	94	90	75	87
B.P.	100	Not testable	65	54
H.S.	102	110	95	95

"t" values for Mean differences	.05 value
Between A & B = 1.16	4.303 Not significant
Between A & D = 2.23	3.182 Approaching significance
Between A & C = 3.72	3.182 Significant
Between B & C = .53	4.303 Not significant
Between B & D = .08	4.303 Not significant
Between C & D = .32	3.182 Not significant

That intelligence is decreased after lobotomy is further substantiated when separate aspects of intellectual function are independently assessed. Halstead and Settlage (7), Porteous and Kepner (14), Goldstein (4), and Robinson (15) have reported greater impairment of some aspects of intelligence than of others. Experience with Canadian war veterans who had been lobotomized suggested to us that there was a characteristic defect in dealing with ambiguous situations for which the subject would have no well-learned response patterns. Accordingly, the Yerkes Multiple Choice Test (16) was modified for simple administration to human patients. Five questions were set. From five plain white cards placed on a table the patient was asked to select what he thought to be the correct one. The examiner arbitrarily chose the left-end card to be correct for the first problem, the right end for the second, the left end alternating with the middle for the third, the right end alternating with the middle for the fourth, and the second alternating with the fourth for the final problem. The subject, by provisionally trying and informed of his correct or erroneous choice on each presentation of a given problem, should

sooner or later settle down to choosing the one or two cards arbitrarily set by the examiner.

Not one of the eight frontal lobe patients examined could complete this test. Only two did the "end" problems; six could do none at all. Such a performance was startling; so startling as to raise the question whether normal people would succeed in completing the test. Seven fourteen-year-old boys of average intelligence and socio-economic background were able to solve all the problems with ease. Of six paranoid schizophrenics of comparable ages, duration of illness, and I.Q., four solved all the problems, while two solved only the simple end problems. Thus the failure by the lobotomized patients on this test would not seem to be attributable to the effect of psychosis on intelligence. On the other hand, failure seems to have been related to certain characteristic behaviour observed in the test performance. For example, (*a*) in the face of failure four of the eight patients yawned frequently and appeared to doze; (*b*) some merely made random choices suggesting that they did not recognize that a problem existed (like Bianchi's monkeys (1) or Jacobsen's Becky and Lucy (2); at the same time they could repeat the directions adequately and appeared to understand what was required; (*c*) some, interjecting irrelevant remarks, seemed to lose their set toward the problem; (*d*) perseverance on the wrong card was frequent. What the deficit is precisely and for how long it persists is impossible to say at this time.

Discussion

The discordant findings in the many studies of intelligence following prefrontal lobotomy suggest that there must be two variables, either one, or both of which, may be affecting the results. The first, alleviation of psychotic symptoms, would make the patient more accessible and, therefore, improve his efficiency on the intelligence test. The second, damage to intellectual capacity, would result in a deficit in his performance on the tests.

Those studies which report no change in intelligence may have examined (*a*) group statistics in which patients with increased efficiency were considered together with other patients with decreased capacity, or (*b*) individual patients in whom the improved efficiency due to the alleviation of psychotic symptoms was offset by decreased capacity due to impairment of intelligence. In each case the effect on intelligence per se, that is, uncomplicated by psychotic performance, would not have been determined, and therefore the conclusion that intelligence was unchanged would be unwarranted.

In those studies in which care was taken to evaluate the effect of the operation on intelligence either by testing non-psychotic subjects or

by obtaining ratings of *pre-morbid* intelligence, impairment was demonstrated. The deficit does not appear to affect all functions of intelligence equally. The impairment becomes more apparent in those situations requiring new complex adjustments, but does not seem to affect adaptations depending on well-learned patterns of behaviour, nor simple tasks involving old skills.

Since this differential effect on intelligence would incapacitate subjects for some occupations more than others, it is incumbent upon those physicians prescribing prefrontal lobotomy to weigh carefully the effect the gains and losses will have, relative to the society in which the patient will later attempt to rehabilitate himself. For the person only mildly incapacitated by the illness, the loss in intelligence will make adjustment to a demanding environment difficult and conducive to relapse; thus the loss will probably outweigh the gain. However, if the environment does not demand complex intellectual function or if the psychosis is severe, or both, the loss in intelligence may be insignificant as compared with the alleviation of distressing symptoms; thus the gain will probably outweigh the loss.

Summary

(1) It is emphasized that evaluation of the effect of prefrontal lobotomy on intelligence must compare the patient's post-operative rating with a pre-operative non-psychotic score.

(2) When this precaution is taken, impairment is apparent.

(3) The deficits appear to be in those capacities required in dealing with complex new situations.

(4) This expected change must be considered in relation to the demands of a particular environment when prescribing lobotomy.

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THE ROLE OF ANXIETY IN INDIVIDUAL FUNCTIONING

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Two current approaches to anxiety as a personality component, that of Freud and that of the social anthropologist Allison Davis, seem to be at opposite poles from one another in terms of the conceptualized relationship of anxiety to personality functioning. Freud sees anxiety as playing a decidedly negative role in individual adaptability ("neurotic anxiety") (4); Davis sees anxiety as providing the major incentive for adaptive behaviour on the part of the middle class child ("adaptive anxiety") (1). A recent study by the writer (5) has yielded evidence that these two approaches to the role of anxiety in the individual personality may not be as contradictory as they appear upon first examination. On the contrary, this evidence would seem to indicate that both are on the same "continuum," with the boundary line between the two "types" of anxiety tenuous indeed.

The writer's study was undertaken in an effort to investigate the effects of two aspects of social training, those of social class and sex role, on the early-adolescent personality. It was based on extensive and intensive case-study materials on thirty children, fifteen girls and fifteen boys, studied from the ages of ten to fourteen (1942-6). These materials were gathered, analysed, and interpreted as part of the "Midwest" Research Project conducted by the staff of the Committee on Human Development of the University of Chicago. The purpose of the project was to investigate character and personality development during later childhood and adolescence; investigation of sex differences was not a purpose of the project. The original sample consisted of all children residing in the small city of "Midwest" who had been born in 1932 totalling 149 in 1942; the intensive case-study sample consisted of thirty-six children representing the two middle and the two lower classes, as defined by Warner's Index of Status Characteristics (5). Nine kinds of data, obtained through the use of twenty-nine instruments, were included in these case studies: interviews, psychometric data, subject reports (checklists), subject reports (free response), ratings, projective techniques (Thematic Apperception Test and Rorschach), sociometric data, physical data, clinical case conference summaries.[1]

The writer's study involved a thoroughgoing analysis of the personality characteristics of those children in the case-study sample whose families' status characteristics placed them in the upper-lower and

[1]The reader is referred to the writer's study (reference 5) for a more complete enumeration and description of these instruments and for a detailed presentation of the methods of selection, procedure, and analysis which she used.

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lower-middle classes (popularly named the "Common Man" level by Warner), 30 adolescents in all. This analysis revealed a personality constellation typical of the entire group, as well as personality constellations typical (in the statistical sense) of the girls and of the boys, respectively. That is, certain consistent sex-group differences were established through application of the chi-square statistical technique. Those characteristics which were possessed significantly more often by one sex than the other, or possessed on a total group basis, were abstracted from each member-child's personality structure.

If at least two-thirds, or 20, of this group of early-adolescents were found to manifest a particular personality characteristic, it was termed "group typical" and, as such, taken to be a function of the training emphases of these children's social status environment. Eleven such characteristics were found.

Five of these *group characteristics* would seem to be strongly corroborative of *Davis's* formulation of the role of anxiety:

(1) The children's level of anxiety or guilt is high or pervasive. As a group, they carry with them feelings of inner tension and apprehensiveness. They are afraid of doing the wrong thing, of being punished for "doing wrong," particularly through having their parents' affection withdrawn. As a result, they have a continual feeling of striving for the "unattainable." These feelings may be attached to specific life situations, such as achievement at school or getting the chores done at home; or they may be diffuse, "free floating," pervasive in character, or both. Or, these inner tensions may be primarily guilt feelings: the children feel guilty for having done something wrong, they have a generalized feeling of guilt, or they are convinced that they are basically "wicked" beings.

(2) Their feelings of anxiety derive from inwardly-felt affectional deprivation. As Davis has pointed out, the giving or withholding of affection is a device used by middle class-oriented parents to "train" their children to be "good," that is, to be obedient, to strive for those behaviours upon which the parents place value. These children as a group, and the girls more so than the boys, have a strong feeling of never having had enough love and affection, a feeling which is a primary source of their anxiety feelings.

(3) They *try* to conform to socially accepted patterns in the expression of their impulses (note distinction from (5) below).

(4) They have an immature, dependent inner life. Their inner life orientation is still that of a child emotionally and intellectually dependent upon parent figures, who are seen as omnipotent, omniscient, the undisputed source of right and wrong, approval and disapproval, succorance and rejection. They have not yet set up their own standards by which to direct their behaviour—that is, they are not self-directing.

(5) A major means of adjustment and of gaining their individual motivations is through active conformity to "middle class guide lines."

However, (2) above, as well as the other 6 group-typical characteristics discovered in the writer's study, may be cited as corroborative of *Freud's* analysis.

(1) Their feelings of anxiety derive from inwardly-felt affectional deprivation. These children as a group, and the girls significantly more so than the boys, have a strong feeling of never having had enough affection and appreciation, a feeling which is a primary source of their anxiety feelings.

(2) Emotional factors deriving from their feelings of anxiety and constriction interfere with their mental functioning (also more typical of the girls than the boys). Their effort to do what is "right," what is approved by their teachers and parents and friends, restricts the creative use of their imagination and their ability to organize concepts meaningfully.

(3) Their ability to organize mentally is poor. Although the children of this group vary in their intelligence test ratings, as a group they have low ability to organize details and unstructured situations into meaningful whole concepts—"low" or "poor" in terms of (*a*) what is clinically considered to be "good" organizing ability, and (*b*) what each child's actual potential in this area is analysed to be by the projective test analyst.

(4) Their imaginal creativity is routine, stereotyped, or little expressed. As a group, these children either make little use of their imagination, or they express it in terms of unoriginal, highly conforming images and ideas.

(5) They indulge in fantasy, fantasy escape, and daydreaming, as outlets for their feelings of anxiety (the girls significantly more frequently than the boys). Reading romantic or adventure "escapist" fiction and dreaming about the future "when things will be better," about having magical powers, about being pretty and popular and the centre of attention (the themes mentioned most frequently) serve to release inner tensions.

(6) They are either unable to relate emotionally to others or avoid emotional ties with others (more typical of the girls than the boys.) Although they have friendships and social relationships, these are not so much on the basis of whole-hearted emotional involvement as on the basis of common interests and activities; they cannot bring themselves to be deeply involved emotionally with anyone (other than their parents), apparently because they feel too deprived of affection themselves to be able to give affection readily to others.

(7) A major life motivation is to obtain more love, affection, admiration from others.

The personality of the typical child of this group, irrespective of sex, may accordingly be described as follows: his inner anxieties interfere with his mental functioning; his ability to organize mentally is poor and he makes little use of his imagination. His immature, dependent inner life makes it difficult for him to form emotional ties with his peers. He experiences high level or pervasive anxiety which stems from his feelings of affectional deprivation. Fantasy and fantasy escape are outlets for these anxieties. Since a major motivation is to obtain more love or approval, he attempts to conform to socially-approved patterns in the expression of his impulses. He finds his chief means of gaining his motivations through conforming actively to the behavioural guidelines provided by his middle-class social environment. Such conformity is also his chief avenue of personal adjustment, of resolving his anxieties.

Certain of the differences between the sexes which involve differential reactions to anxiety may also be classified as "adaptive," or "non-adap-

tive" or neurotic in trend. The following are certain relevant *personality characteristics typical of the girls* of this group, as compared with the boys:

"Adaptive" (Girls):

(1) If an achievement drive is present, its chief source is a desire to conform to parental and/or general social mores. For those girls possessing a drive for achievement in the academic area, a major motivation for this drive is the wish to conform to parental demands and to the value system of the school itself.

(2) They find the accomplishment of set tasks satisfying. It relieves their inner tensions to start and complete simple, routine tasks such as cleaning their rooms, putting the house in order, or doing their homework.

(3) Their school achievement is up to or higher than their tested ability.

(4) A major means of adjustment and of gaining their individual motivations is through active conformity to the middle class guide-lines of their social environment. They have found that if they do what their parents and other adults tell them to do, as well as those things within the peer group that their friends approve of–if they are "good," if they work hard in school, if they dress neatly, if they behave properly when adults are around, if they act like their friends–they not only get the approval they need, but they are also able to reduce their inner concerns and tensions and feel happier.

"Non-adaptive" (Girls):

(1) They accept their impulse life inwardly, but do not allow it outward expression. That is, they habitually express their feelings, emotions, desires only on the inner, covert level, but suppress the overt, spontaneous expression of their impulses, particularly their agressive and sexual feelings.

(2) They manipulate others in their social environment as a reaction to their feelings of anxiety. Their feelings of affectional lack, fear of disapproval, need for admiration, motivate them to manipulate others (particularly those their own age and younger) to do things that will lead to their obtaining the admiration and approval from adults and peers that they need. For example, a girl may consciously act wistfully and fearfully in order to be treated protectively and lovingly; another girl may make a practice of breaking off a close friend's other friendships in order to have the friend "to herself."

(3) Paralysis of action, that is, overly rigid outer control, is a frequent reaction to their feelings of anxiety. Occasionally they become so tense and fearful lest their inner feelings of strain and tension break through to observable behaviour that they are rendered incapable of taking any action on their problems at all; their outer control of their emotions becomes so suppressing as to be rigid.

(4) Feelings of anxiety derive from conflict between inner and social demands. What they would like to do often conflicts with what others, particularly authoritative adults, expect them to do and insist that they do. For example, although they would like to express their aggressive impulses freely, they know that adult approval will be withdrawn if they do so; the ensuing inner conflict engenders tension.

(5) Feelings of discouragement and worthlessness (intrapunitive tendencies) are present as a reaction to their feelings of anxiety. They believe themselves to be inadequate, ineffective beings, unworthy of being loved, as a result of their feelings of affectional lack and fears of failure.

(6) They use fantasy and fantasy escape as outlets for their anxiety feelings. Reading romantic "escapist" fiction and dreaming about the future when "things will be better," about having magical powers that can annihilate disliked persons,

about being pretty and popular and the centre of attention (the most frequent themes mentioned in the case materials), serve to release inner tensions.

(7) They are either unable to relate emotionally to others, particularly peers, or they avoid emotional ties with others. Although they have friendships and social relationships with others, these are not so much on the basis of emotional involvement, as on the basis of common interests and activities: they cannot bring themselves to be deeply involved emotionally with anyone (other than their parents), apparently because they feel too deprived of affection themselves to be able to give affection freely to others.

(8) Emotional factors due to their drive for conformity interfere with their mental functioning. Their effort to do what is "right," what is approved by their teachers and parents and friends, restricts the creative use of their imagination and ability to organize concepts meaningfully.

(9) Feelings of anxiety derive from inwardly-felt affectional deprivation. They feel they have never had enough love and affection from their parents and these feelings of lack contribute to a continual state of inner tension of varying degrees of intensity.

The relevant *personality characteristics typical of the boys* of this group as compared with the girls are as follows:

"*Adaptive*" (Boys):

(1) A source of guilt or anxiety feelings is their concern over their lack of impulse control. They are aware that their impulsiveness is not adult-approved and, as a result, they feel either guilty or anxious.

(2) A major motivation is the desire to know how to control their impulses.

(3) They are in an active process of developing a self-directive system of impulse-control; that is, they are aware that controlling their impulses must be their own job and they are working towards achieving it.

(4) They have sensual self-control outlets in addition to fantasy for their inner feelings of anxiety. A means of compensation or adjustment for them is the adoption of activities which afford sensual gratification, such as excessive eating, body-contact sports, certain hand-skill hobbies, swimming, excessive verbalism, raising animals.

"*Non-adaptive*" (Boys):

(1) Their overt behaviour is a reactive, covering-up "front" for their anxiety feelings. They adopt "tough guy" behaviour patterns as a defense measure, as a means of hiding their inner anxieties, guilt feelings, and affectional needs.

(2) Their school achievement is below their tested ability.

The presence in these "normal" children of a high or pervasive level of anxiety which stems partly from inner feelings of affectional deprivation, anxiety which pushes them to try to conform to socially-expected patterns in the expression of their impulses and to use such active conformity to middle class guide-lines as an adjustive (anxiety-reducing) and reward-gaining mechanism, would seem to add further independent evidence of the type which led Davis to his postulation of adaptive anxiety. It is also interesting to note that the boys of this group tend to discharge their anxieties in more personally adjustive ways than do the girls. This finding strongly suggests that the "Common Man" social

level provides a greater number of adjustive behaviour outlets for the male than it does for the female.

However, neurotic anxiety as described by Freud also seems to be present in these children, particularly in the girls. If Freud's concept of "sexual restriction" is broadened to "affectional restriction," which is currently a commonly-accepted interpretation among students of personality, a major genesis of anxiety in these children is found to be affectional deprivation with certain negative effects on their functioning. That is, to recapitulate, feelings of affectional deprivation have led to a high or pervasive level of anxiety which interferes with their ability to organize mentally as well as they might, reduces their imaginal creativity, has outlets in fantasy and daydreams, makes it difficult for them to form positive emotional ties with others even while their feelings of deprivation make the formation of such ties a major life motivation. Freud, himself a product of middle-class Vienna, apparently did not recognize that withdrawal of love and approval was a "training technique" employed consistently by the parents of his patients, and was, as such, a middle class culture pattern.

An attempt was made by the writer to explore whether any of five environmental and biological variables might be related to the extent of sex-typicality and group typicality among these children.[2] The only variable which seemed to show any consistent pattern of relationship with the highly group-typical children was this kind of home environment:

> Child-rearing is interpreted as a moral duty by these parents. It is their duty to raise "good" (obedient) children and to provide them with adequate physical care. For their part, the children accept that to be a good child is a question of moral duty. Thus, to disobey their parents is to break a moral law. The family is a closely-knit unit, whether or not the mother works. Most of the children's activities take place within either the immediate family circle or the extended kinship circle. The family's community ties are usually church-centered.
>
> The mother shows the child little warmth or affection; the father backs up the mother in her disciplinary role. The training patterns emphasize the repression of impulses, especially the sexual, the importance and necessity of achievement and conformity, strictness of enforcement of early-childhood training routines. Hard work is seen as a moral duty and is interpreted within the family by the assignment of home tasks to each child. Punishment is administered in a non-hostile, almost routine, manner. The approval of parents (and teachers) is the child's reward for conformity and obedience. The mother-child relationship may be characterized as under-loving and over-controlling.

No contradiction or enigma need be perceived in the presented evidence. It would seem that the social inculcation of anxiety does not, in the normally-functioning adolescent, result in the generation of "just

[2]Upper-lower and lower-middle class status, I.Q., maturation level, adjustment rating, home environment.

the right amount" of anxiety to ensure socially-adaptive striving. Evidently "excess" anxiety is generated by the individual's social training and *as such* is neurotic in character. Apparently, the group of children studied have had to bear a good deal more anxiety than serves to make them "strive more effectively for the goals of their group." It would therefore appear that neurotic anxiety differs from adaptive anxiety in degree rather than in kind. It is interesting to note that the girls of this group seem to be more adaptive in Davis's sense than the boys, but that they seem to manifest at the same time more non-adaptive or neurotic characteristics.

But the dilemma occasioned by an apparent conceptual contradiction between neurotic and adaptive anxiety has been replaced with a dilemma of a different nature. From the cited evidence, it would seem to be a logical conclusion that the social inculcation of anxiety in individuals for socially-adaptive ends leads at the same time to the inculcation in them of anxiety which is non-adaptive or neurotic in character. This question comes immediately to mind: is the latter an *essential* concomitant of the former? Or, to put it another way, must the individual pay a high personal price for the maintenance of socially-adaptive behaviour patterns in our society? Is there any possibility that social training could be so adapted that enough anxiety to maintain striving would be instilled, but not so much that neurotic anxiety would also result? Suppose such training techniques as withdrawal of affection were not begun in infancy, but rather, techniques involving the symbolic or verbal levels of communication were applied *after* infancy and early childhood, after the child has acquired some capacity to be "objective" and to be aware of the meaning of such verbal training techniques. Would this approach minimize "repression into the unconscious" and hence lessen the generation of neurotic anxieties, or would it also, perhaps, result in a diminution of adaptive anxiety as well?

Another dilemma would seem to arise from the circumstance that adaptive anxiety as conceptualized by Davis serves a socially useful function, while the neurotic anxiety which has been found to be its concomitant appears to lead to socially negative outcomes. For example, the girls of this group have been "trained" to feel inferior, to repress outward emotional expression, to find positive action difficult, to perceive their social environment as unfriendly, even threatening, to manipulate others to satisfy their own needs, to have difficulty in forming close emotional ties with others. Are these a constellation of traits which make it likely that these girls will eventually be adequate wives and mothers?[3]

[3]The "Common Man" social level was found to constitute 72 per cent of the population of "Midwest"; Carson McGuire has estimated from 1940 census data

A further example of the high price our society would seem to be paying for many of its child-rearing practices is contained in recent research on the nature and genesis of the "prejudiced personality" (3). A constellation of personality characteristics associated with a high degree of measured prejudice was derived from a group of 60 children ranging in age from 11 to 16, in comparison with a group of 60 children who had shown a low degree of tested prejudice. The "prejudiced personality constellation" and the nature of the family environment found to be associated with this constellation are strikingly parallel in many aspects to the findings of the writer's study. Unfortunately, the families of the California study were not class-typed according to the Warner I.S.C. formulation. There are many indications in the data presented, however, that the parents of the prejudiced children were predominantly of the "Common Man" social level and that the parents of the "liberal" children were chiefly of upper-middle-class status.

A study by Evelyn Millis Duvall has documented the relationship of predominantly "traditional" child-rearing approaches to lower-class status, and the relationship of predominantly "developmental" approaches to the financially and socially secure upper-middle status level (2).

It has become "fashionable" in recent years to indict parents as the major culprits responsible for their children's maladjustments or "anti-social" behaviour. The evidence here presented suggests a far broader and more complex interplay of related environmental factors. The child-training patterns practised by parents must be seen as essentially a reflection of the value-systems of the social group and the over-all culture of which a particular family is a part. An important question for social-psychological research accordingly becomes: what is the nature of the social pressures which have influenced these parents of the "Common Man" social level in their child-rearing practices? That these pressures are complex cannot be overlooked, but at least one promising direction for further investigation is suggested by this observation in Dr. Brunswik's study: "The parents of the ethnocentric children are often socially marginal. The less they can accept their marginality, the more urgent becomes the wish to belong to privileged groups. . . . With this narrow and steep path in mind, such parents are likely to be intolerant of any manifestation on the part of their children which seems to deter from, or to oppose, the goal decided upon. The more urgent the social needs of the parents, the more they are apt to view the child's behaviour in terms of their own instead of the child's needs" (3, p. 302).

It would seem to be legitimate, in the light of the foregoing evidence, to rephrase the dilemma occasioned by the contradictory "social roles"

that the upper-lower class constitutes 40 per cent of the population of the United States and the lower-middle 30 per cent.

of adaptive and of neurotic anxiety as follows: Is our complex, status-valuing and seeking, yet increasingly less socially-mobile American society having to pay too high an individual and social price for its own self-perpetuation?

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NEW APPROACHES TO STUDIES IN EDUCATIONAL LEADERSHIP

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Our history is so yawed by the disparity and convergence of social forces that some may conclude it evolves in a totally capricious and unlawful fashion. However, good hindsight has generally been able to detect the origins of this or any other hour somewhere in our history. Long range social progress is in no small part dependent on the dynamic agency of man, and the direction of this progress is dependent on values held commonly by men and their leaders. It is, furthermore, highly improbable that any leader can for long expend the social energies without public approval, and it is less probable that people will easily expend themselves for ends in which they do not believe. These humanly desired ends become the objectives of education. In fact, much of the justification for the school lies in our faith in the improvability of man and his environment.

If the school is to serve a creative function in progress, there must be education for the leadership of men. Nowhere is this need more vital than in teacher education. New College, Columbia University, recognized a primary function in assisting students "to develop their own capacities for intellectual and social leadership" (16, 3). Teacher education programmes have recently emphasized educational sociology. This emphasis would seem to arise from a growing recognition of the educational function in social evolution. An effective teaching force must be characterized by social leadership and more imminently by democratic classroom leadership.

Research on leadership has been concentrated in the area of prediction. Most investigations have correlated achieved leadership and personal traits; few have investigated the pattern of circumstance in which the position was developed. The educational problem is complex in that it goes beyond prediction to preparation for leadership—leadership of a kind suited to democratic philosophy. The concept of differentiation among types of leadership is not new (4). It is clear that "hypnotic" and "imitative" leadership is not in keeping with the purposes of a dynamic and creative society. Neither may we accept a theory of leadership as a dominance which becomes repressive and despotic. Rather than exploitive leadership, the demands of democracy are for direction characterized by social sensitivity and the sharing of common interests and common concerns. Studies by Lippitt and White identify a type of leadership that is democratic and results in democratic behaviour (2, 485-508). This

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and other contributions have begun the task of definition of desirable techniques and perhaps assist in identification of students who have significant leadership possibilities.

Two types of study have made examination of the basis of leadership. The first of these has been essentially a rating scale method with its problems of definition of terms and unreliability from personal standards and interpretations. The classification of traits such as "fairness," "self-sacrifice," and "moral-ascendency" have characterized military investigations (3). Not only has there been inadequate validation of the instruments (8, 64), but the concept of military leadership is perhaps not directly applicable to school leadership. A second area of enquiry has related physical and mental factors with leadership. Many studies, including those of Parten, Partridge, Terman, Hunter, and Jordan, discovered tendencies for leaders to be taller (8), to have fewer physical defects (7, 51), and to be slightly above the mean intelligence for that group (10). There is, however, no assurance, for example, that physical prowess which may be a major factor in leadership of adolescent boys is equally important in girls, or in young boys or in men. Thrasher (17) and Tryon (2, 545-65) discovered that criteria for judgment of appearance and other factors change with maturity. Undoubtedly the criterion is altered with social class, geographic location, and experiential background. The group may change its basis of approval, or an individual may alter his relative position in height, physical defects, or achievement, and therefore this type of study has major significance for a specific group under specific conditions.

Group work theory has considered factors sometimes overlooked in highly academic approaches. The theory has perhaps been more concerned with the function of the follower as part of leadership. Jennings found that groups seek independence unless membership is based on participation, responsibility, and the right to appeal (12). This point of view has eschewed moralistic and exploitive control (12, 532-40) and has maintained that leadership arises in genuine welfare considerations.

Within this thesis a second characteristic of leadership is an integrative quality which includes responsive understanding, ability to assist in constructive group expression, and the presentation of self as a unifying factor. A study of leader-follower relations in street corner society shows a system of "mutual obligation which is the fundamental to group cohesion" (12, 403). The investigator, Whyte, concludes that the "why of leadership is not in any quality or constellation of traits," but rather, "The why of leadership appears to reside in the interpersonal contribution of which the individual becomes capable in a specific setting, eliciting such contributions from him" (12, 412). Sanderson (14) outlines a theory of leadership in which power is derived from collective support

of a collective objective, and such reciprocal leadership is in keeping with educational purpose.

Leaders not only symbolize the common cause but make contribution in the specifics (14, 27-39). Whether they are the choice of the group or the creator of the group, their leadership has origin in the face of a new situation. It is thus that world frontiers, with their mobility and equality, provide striking examples of evolving leadership. Where there is danger or progress, the leader has a specific active function. To serve this function, a leader must have specific abilities which may explain his general superiority in intelligence, ability, and appearance (15, 69). Leadership is adaptive behaviour and the duty falls to one who can make a contribution. This implies that training for leadership includes understanding of social structures and processes, and development of specific skills and techniques.

A fourth characteristic of desirable leadership is the evolution of continued leadership. One of the most remarkable movements in recent times, the co-operative associations of fishermen and miners in Nova Scotia, developed by the Fathers Coady and Tompkins at St. Francis Xavier University, is built on common purposes and belief in leadership from the people. The vigour and vitality drawn from the newcomer is the secret of continued growth and strength in democratic organization.

Finally, education and group work endorse the same method of leadership. The Lippitt and White studies are evidence in the contention that certain leader techniques are productive of democratic behaviour. Public agencies may not abandon the methods of persuasion and example for those characteristic of "power-driven" leadership.

To reiterate, democratic leadership arises from accepted common purposes, interdependence of achievement, specific contributions in the face of a problem, the evolution of new leadership, and the acceptance of agreed method. Reciprocal leadership depends on an interchange of identification where there is leader-follower concern to act in common purpose. Ordinarily, the leader may be somewhat in advance of group thinking and that may be his contribution, but always he must be able to synthesize his conception with the popular conception of the objective. If we accept this theory, then "values" are basic in leadership.

The outstanding study of military leadership is in agreement on this point. Guided by the "Harvard Study," Murray and MacKinnon made contributions in the area of situational tests and evaluation of leadership as it operated for a specific mission (11). In measuring a man for a job, O. S. S. found they must know what he believes, hates, thinks, and feels (5, 92)—they must know his "values."

A recent intensive study of thirty-three teacher trainees at Stanford University bears out the same conclusion (6). Various factors were

related to educational leadership as estimated from extensive observational records. The observational instrument accepted major values in social climate, belongingness, and qualities inherent in reciprocal leadership. Among factors investigated for relationship were: appearance, ranking, professional information, academic success, personal vital statistics, understanding of children, knowledge of agencies, information concerning specific children, group leadership experience, cultural activities, work experience, objective personality measures, and attitude-interest patterns as measured on the Allport-Vernon *Study of Values* (1).

Many of the measures including the personality factors had no discovered relation with the quality of group leadership. The *Study of Values,* however, was significantly predictive and the pattern of relation was more productive of discovery than the degree of relation. The correlations with performance as discovered by the "Spearman Rank Difference" method were of the order:

1.	Theoretical	−.345
2.	Economic	+.025
3.	Aesthetic	−.348
4.	Social	+.269
5.	Politicai	−.011
6.	Religious	+.628

The "economic" value is associated with a desire to surpass others; the "political" value is associated with a purpose to dominate others (1). Both are characterized by the power motive which, though sometimes considered essential in leadership, is foreign to the mutual obligation basis of reciprocal leadership. The "theoretic" and "aesthetic" significantly negative relation may reflect an individualistic detachment that denies the possibility of integrative service. Thus the "value" categories with no significant relation and those with a negative significant relation to leadership performance contribute to the interdependent theory.

Further support is provided by the significant and positive relation of "social" and "religious" values. Kilpatrick (1, 118) maintains that a leader is responsible for choice and purpose as well as guidance, and it would seem that these "values" would be essential in either choice or purpose that seeks follower welfare. The very significant relation of the "religious" category might be interpreted as indicative of the importance of purpose in those who lead. It is the contention of Slavson that a major area of training for leadership is in the formation of attitude and in the above study there is some evidence of the need for social understanding and philosophic purpose.

Freehill, in the study at Stanford, outlined the need to determine the level at which these factors operate (6, 136). No one in this study who

ranked in the upper quartile of "religious values" was in the lowest quartile of leadership performance, while a subject in the first quartile of "economic" interests stood a two-to-one chance of rating in the lowest quartile of the observational record. A problem remains for investigation with a larger sample. Is there a critical range—between two scores—above a minimum—or below a maximum?

It is rather commonly agreed that social drift is inadequate assurance of progress.

> If our colleges and universities are to graduate individuals who have learned how to be free, they will have to concern themselves with the development of self-discipline and self-reliance, of ethical principles as a guide for conduct, of sensitivity to injustice and inequality, of insight into human motives and aspirations, of discriminating appreciation of a wide range of human values, of the spirit of democracy, compromise, and cooperation (13, 10).

Leaders are a social device deriving authority from ability to contribute to group welfare. When political surprise results from a popular ballot, we tend to seek an explanation in method rather than platform. It is probable that the electorate are allied with the position of the candidate as they interpret that position. The campaign methods, of course, may be the symbol on which the interpretation is made. With a base of either the school or society, there is some evidence that leadership factors are more closely related to principle than to method.

Education may never choose to overlook development of purposes and principles since these are basic in the personal fullness of maturity. Beyond that, the principles that are taught may determine the nature and direction of our social progress. This hypothesis predicates that we should examine the relation between leadership and values.

Research in the area of "values" as prediction and operating factors in effective teaching may supplement and interrelate the contributions of previous investigation. These investigations have centred on the separated areas of predictive leadership traits and desirable teaching technique. Research is needed to discover which values held by students are predictive of successful teaching careers. Research is needed to discover which values held by practitioners are indicative of effective techniques. Research is needed to discover which educational experiences will produce growth of value patterns predictive of and indicative of excellent teachers. Research is needed to examine the hunch that "values" are the basis from which leadership evolves, and that "values" serve as a source of energy and direction in educational leadership.

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BOOK REVIEW

Organization of Behavior: A Neuropsychological Theory. By D. O. Hebb. New York: John Wiley, 1949. $4.00.

Although the subtitle of this work might suggest that it is a book of limited interest, written for specialists in the area of physiological psychology, it is in fact a text in which all psychologists can find enjoyable, stimulating reading. The reviewer feels that the following features of the text give it general appeal in addition to its obvious significance to the physiologically oriented worker. The book develops a provocative and promising neuropsychological theory of behaviour which, although lacking in some details and probably wrong in others, nevertheless should serve as a potent stimulant to critical thought and research in many areas of psychology. In developing the theory, Dr. Hebb provides a review and interpretation of a wide range of psychological and neurological literature. To this review is added an incisive analysis of our present state of knowledge in areas of innate behaviour, attention, perception, emotional and intelligent behaviour, learning and mental disorder. This analysis in turn leads to suggestions for research.

To the first point, Dr. Hebb feels that recent neurophysiological findings make possible a reasonable revision of the currently discarded notions of synaptic change resulting from neural activity. The development of this idea leads to the postulation of two types of learning: early and late. Early learning is slow, involving structural and/or functional changes in the transmission characteristics of neurons. Neurons associated with a situation-reaction become organized into functional groupings of cells which act briefly as a closed system. This stage is called the development of "cell assemblies."

As this stage of development proceeds, the organization and reorganization of groups of cells becomes increasingly possible with the attendant behavioural manifestations of large transfer effects, rapid or one trial-learning, and greater complexity of behaviour. This time ordering of cell-assemblies is called the establishment of "phase sequences." There is an interesting parallel between this conception and the Wheeler-Snoddy hypothesis which relates learning to maturative and growth processes. Certainly these ideas deserve more consideration than they have received to date; perhaps with the support of the present theory a more adequate experimental evaluation can be achieved. This conception of two stages of learning relates also to the various "two factor" theories of learning. At least one unique feature of Dr. Hebb's development, in addition to the neurological orientation, is his emphasis upon a developmental analysis. He feels that many writers and investigators in the past have failed to recognize the role of early learning in the eventu-

al development of perceptual and emotional reaction tendencies and have accordingly overstressed the innate, unlearned quality of this behaviour.

Another historical controversy in which Dr. Hebb takes sides is the central-peripheral argument. He feels that the weight of evidence supports the importance in the organization of behaviour of central processes and states. The role of the physical stimulus and its attendant sensory processes are of lesser psychological significance than central activities conceived in terms of the organization and functioning of cell assemblies and phase sequences. The developmental level and present activity of the brain are significant, often dominant features in the organization of behaviour.

Even the reader possessing a minimal interest in neurophysiological theory will find *Organization of Behavior* a useful reference work to a wide area of psychological thought. The book is organized so that the person less interested in the evidence on which the theory is built and more interested in the proposed applications of the theory to the broader field of psychology can omit Chapters 1 through 5 without serious loss. The book provides an excellent synopsis of a considerable literature. It is well indexed and contains a 285-item bibliography which the serious reader will probably need to use unless he is exceptionally well read. Dr. Hebb in his discussion of the literature assumes a considerable sophistication on the part of the reader, citing only the most important or relevant features of studies which he uses in the development of his theory. If the reader accepts the challenge, he can find this book a searching test of the adequacy of his knowledge of systematic and experimental work and thought.

In psychology we have come to appreciate the value, indeed the necessity, of formulating our hypotheses in testable form. In this text there is a constant effort to relate theory to evidence and to suggest ways in which evidence might be obtained when it is lacking. An outstanding feature of the book is the problems which it highlights and the research programme which it suggests. It is a veritable gold-mine for the problem-seeking Ph.D. candidate. To provide a single example from many:

> I believe that such conceptions will be a good basis for the study of motor learning, but it should be emphasized that the study is still needed—that nothing more has been provided than a rational approach which may or may not be found adequate in the long run. The discussion has said nothing about the changes of skill with practice. What is their nature? Are they to be comprised in the conception of changes in the cell assembly due to an increased frequency of activity? Does the subject revert to his first clumsiness of performance when he is required to use a new muscular pattern? Does his perception of the situation change with practice, or his muscular timing—or both? Above all, is he still responding to the same cues? (p. 157).

Organization of Behavior is a book that cannot be exhausted in a single, even careful studied reading. For many psychologists it should prove to be a useful reference work. To many more it offers an exciting adventure in reading. Dr. Hebb has something important to say and he says it in a most interesting, sometimes almost conversational style. Canadians may well be proud of this excellent work and of its distinguished author.

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ANNOUNCEMENT

PRINCETON, N.J. The Educational Testing Service is offering for 1951-52 its fourth series of research fellowships in psychometrics leading to the Ph.D. degree at Princeton University. Open to men who are acceptable to the Graduate School of the University, the two fellowships each carry a stipend of $2,375 a year and are normally renewable. Fellows will be engaged in part-time research in the general area of psychological measurement at the offices of the Educational Testing Service and will, in addition, carry a normal programme of studies in the Graduate School. Competence in mathematics and psychology is a prerequisite for obtaining these fellowships. Information and application blanks may be obtained from: Director of Psychometric Fellowship Programme, Educational Testing Service, 20 Nassau St., Princeton, N.J.